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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Black Carbon Seasonal and Diurnal Variation in surface snow in Svalbard and its Connections to Atmospheric Variables" *by* Michele Bertò et al.

Anonymous Referee #1

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This study aims at linking a number of observed variables with the concentration of rBC in surface snow. The purpose is to investigate processes regulating rBC mass concentration in snow by using a multi-linear model to explain the observed variability of rBC in surface snow.

In general, the manuscript show some signs of being rushed to submission, yet it would benefit from substantial trimming. Specifically the introduction could be shorter, the speculations could be fewer, and material related to trajectory calculations do not forward the scientific reasoning and can be omitted from the study.

The opening statement in the conclusions illustrates some of the problems mentioned

above "....the main drivers of the rBC mass concentration variation in the Svalbard surface snow are mainly precipitation events, snow metamorphosis (melting and surface hoar formation and sublimation), and potentially the activation of local sources during the melting periods triggering a positive-feedback based on the snow albedo reduction".

The first section could very well have been stated before the study and the proposed potential activation mechanism (assuming that "local sources" refers to dust) is never associated with any observed albedo reduction, nor snow temperature increase.

In its current form I cannot recommend publication of this manuscript and it requires substantial revision before it can be considered again. My main reason is the method used, which is the linear model based on essentially instantaneous observations, which may or may not be relevant.

In a column of snow there are not so many ways to change the concentration of BC. Assuming that BC is not removed from a column of seasonal snow (other during catastrophic melting events or special cases), then BC can only be added whereas water can be both added and removed. Over a season, the concentration of BC is not very much determined by the momentary environmental parameters, but rather the integrated deposition of BC (wet and dry) and the integrated flux of water. Hence, what shows up in late spring is the integrated contribution of the processes throughout the season. To get closer to a useful model, the time series could be transformed into cumulative variables where snow events are treated separately (as it covers up old snow and in a way puts some processes on hold).

My point is that the variables and method is not appropriate for the scientific question. On a daily frequency (even more so on an hourly basis) there is no real reason why the atmospheric concentration and the concentration in 10 cm (or even 3 cm) of snow should be related. The exception is during a precipitation event (cf. Hegg et al., 2011). Dry deposition is too slow and the volume of air above is too large for any observable decisive impact on daily basis. For instance; to change the average concentration by 1 ng/g in 10 cm of snow at a density of 0.5 kg/L, an atmospheric concentration of 50 ng/m^3 and a Vd=0.3 mm/s (see line 125 in manuscript) will take about 2 months. The fact that dry deposition contributes to 100% of deposition between snow events does not make it very important on a seasonal time scale except in very arid regions (cf. Wang et al., 2014).

In spring, a round figure for "non-new" snow density could be 0.5kg/L, this makes 10 cm represent approximately 50 mm melted equivalent water. This number should be compared to the annual precipitation of approximately 380 mm. In other words, each daily sample of snow represents the same order of time as the whole 80-day campaign (give or take).

Based on this comparison above, one would not expect a large daily variation from samples taken almost side-by-side. However, my interpretation of the data is that there is indeed a strong signal of this variability in the data. See for instance Svensson et al. (2013). The authors reference Spolaor et al. (2019) and claim comparable spatial variability of 5-15%. But, the species observed in Spolaor et al. (2019) have very different chemical and physical properties compared to BC.

I agree with the authors that minerals and BC are likely to have similar properties, which eventually enhance their surface concentrations. This in combination with the spatial variability give these two variables the strongest link of the parameters tested in the linear model. The process most likely responsible for generating the observed spatial variability is snow drift (this process is not discussed by the authors). Sastrugies (that often form on the surface of snow from wind) are in the current context a travel in time and a process to generate small-scale variability of many snow impurities.

Instead of SW radiation and snow temperature (not entirely independent, as pointed out by the authors), it would be more appropriate to use the difference in water vapour pressure between the surface snow and the air above. This is readily estimated from

the snow temperature (outgoing LW can also be used) and RH and air temperature. This will explain the effect of hoar and evaporation on changes in BC concentration. These processes have both a seasonal and diurnal cycle, but the order of magnitude is typically less than 1 mm equivalent melt water per day. Again, this value should be compared to the amount of snow sampled each day in the two campaigns. Also, working with the difference in water vapour pressure will take care of the apparent time lag noted in Figure 3 by the authors.

In spring, most processes drive the concentration of BC to increase. The big exception is snow events, which will, with very few exceptions, reduce the surface concentration of BC where it fell. The fact that the authors appear to see an increase of BC especially just after a snow event could be an effect from the tractors clearing snow from the roads nearby the sampling site (cf. Figure 1a in manuscript and https://www.youtube.com/watch?v=2Y1YZONbfFY for a perspective).

There are minor questions that can be asked on details, but I feel there is a need to substantially revise the manuscript with a more clear idea about the relation between temporal and special scales and what processes (variable links) that are plausible in regulating rBC in surface snow. Perhaps my order of magnitude estimates above are wrong, but I encourage the authors to do similar assessments, nevertheless.

Hegg, A.D.,et al. Measurements of black carbon aerosol washout ratio on Svalbard. Tellus B, 63, 891-900, 2011. Svensson J. et al Observed metre scale horizontal variability of elemental carbon in surface snow 2013 Environ. Res. Lett. 8. Wang, ZW., et al., Elemental carbon in snow at Changbai Mountain, northeastern China: concentrations, scavenging ratios, and dry deposition velocities. Atmos. Chem. Phys. 14, 629-640, 2014.

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